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LORD KELVIN'S PAPERS.

Mathematical and Physical Papers by the Rt. Hon. Sir William Thomson, Baron Kelvin, O.M., P.C., G.C.V.O., &c. Arranged and revised with brief annotations by Sir Joseph Larmor, Sec. R.S., M.P. Vol. iv., "Hydrodynamics and General Dynamics." Pp. xvi+563. Vol. v., "Thermodynamics, Cosmical and Geological Physics, Molecular and Crystal-line Theory Electrodynamics." Pp. xv+602. (Cambridge University Press, 1910-11.) Price 18s. each.

THE publication of volumes iv. and v. of Lord Kelvin's "Mathematical and Physical Papers," bringing as it does the collection of these memoirs nearly to an end, is important both to the student of mathematical physics and to the scientific historian. It gives the former access to papers of great value scattered through the Transactions and Proceedings of learned societies, or imbedded in the more easily obtainable but also more unexpected pages of the journals of pure and technical science. The latter will seize the opportunity to compare the utterances of the master during the last decade or two of the nineteenth century and the first seven years of the twentieth, with the last developments of the related theories of electrical action and the ultimate constitution of matter. To those who have come to these developments from the point of view of the Maxwellian electromagnetic theory of light, and find in atomic facts and hypotheses a supplement and to some extent a basis for the undulatory propagation of electric and magnetic action in a non-conducting and uniform medium, some of these utterances will occasion much thought, if they are not, indeed, a source of a good deal of perplexity.

Consider, for example, the Appendix A of the "Baltimore Lectures," and its continuation in volume iv. of the "Papers." One statement is enough to cite: "Stress in ether, being thus freed from the *impossible task of transmitting both electrostatic and magnetic force*, is (we may well imagine) quite competent to perform the simple duty of transmitting magnetic force alone." (The italics are in the original.) The Maxwellian electrician has the notion ingrained in his mind that, when magnetic force is propagated in the medium, electric force is propagated also as a necessary concomitant, and *vice versa*, and therefore supposes that whatever machinery is effective for the one must be equally concerned with the other. Moreover, as Lord Kelvin states in a note to the first article of this appendix, the "so-called 'electromagnetic theory of light'" is to be grafted on to the elastic solid theory of the æther. It is, however, to be remembered that Maxwell came to his theory by way of Faraday's experimental researches and Lord Kelvin's mathematical interpretations, and it may be that these utterances, dark as they may appear to some of us, will in time to come throw light on some of the still unsettled questions of electricity.

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The student of these papers has a difficult task; but it is one which, if it be not thrown up in despair, will stimulate his power of independent criticism of physical doctrine.

The arrangement of the papers is both chronological and according to subjects. Hence volume iv., which is mainly devoted to hydrodynamics, goes back to the vortex-motion papers of 1867 and 1869. The first of these contains the famous suggestion that the vortex-rings of Helmholtz are the only true atoms. Their non-creatability in a perfect fluid does not seem to be regarded as a difficulty, but rather as a recommendation—a creative power above nature brings them into existence, and once they are in existence, their indestructibility by natural agency is assured.

It is rather remarkable that Helmholtz's great paper of 1858, on the integration of the hydrodynamic equations of vortex motion, should not have attracted the active attention of physical mathematicians in this country at an earlier date than 1867. In that year Tait published his translation of the memoir, and made experiments on vortex-rings rendered visible by smoke in his class-room. It was seeing and, so to speak, handling these rings that suggested Kelvin's conception of the vortex-atom constitution of matter, and, with Tait's account of Helmholtz's investigations, led to the paper on vortex-motion in the Edinburgh Transactions for 1869, which will ever rank as one of the great classical memoirs of hydrodynamics. In this Kelvin's point of view and method are curiously different from those of Helmholtz. The results of the latter are derived in the most approved classical manner from the equations of motion of a perfect fluid, unsimplified by the hypothesis of a velocity-potential. It is straightforward mathematical discussion; but the march is that of a clear-eyed intellectual giant, and the stages reached are duly chronicled and described in language which shows how completely the author understood the physical meaning of his mathematical results.

Kelvin, on the other hand, starts from the physical conceptions of linear and angular momentum, and from these ideas, and the impulses required to produce the momenta, the whole subject is developed. The physical meaning of the expressions $\xi = \frac{1}{2}(dw/dy - dv/dz)$, . . . , namely that, multiplied by the moment of inertia about diameters parallel to the axes of coordinates, they are the component angular momentum of an infinitesimal sphere of the fluid with its centre at the point x, y, z , had been given by Stokes in 1845, and thus the existence of a velocity potential the criterion for which is the vanishing of ξ, η, ζ , had been shown to affirm the non-existence of angular momentum in the fluid to which the criterion applied. This physical interpretation is referred to and used by Kelvin, and in a sense is the keynote of his discussion of the subject.

Perhaps the most important of all the results in the memoir is the theorem of Thomson regarding "circulation"—that is, the line integral, along a specified curve or line of the fluid, of the compo-

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nent of flow along the curve at each element. If the path be not closed the time-rate of growth of the flow along any specified path which moves with the fluid is obtained by subtracting the value of the excess of the kinetic energy per unit mass of the fluid, supposed incompressible and under no applied force, above the pressure at the initial end of the curve of integration, from the value at the terminal end. This theorem, in its modification for a compressible fluid in which the pressure is a function of the density, and which is situated in a field of applied force, has been truly said to comprehend the whole of the dynamics of a perfect fluid.

For the vortex-motion application the use is immediate. If the curve be closed the time-rate of growth of circulation for the moving circuit is zero; but the integral which is thus shown to remain constant is half the surface integral of the component of ξ , η , ζ , along the normal to each element of a surface of which the curve is the bounding edge. Hence as the fluid moves this surface-integral remains constant, and if once zero is always zero.

Thus the Kelvin theorem obtains the result for a finite mass of the fluid that is obtained in another way by Helmholtz, and it gives the subject a physical significance at once clear and convincing. The theorem may, however, be made to give at once many well-known results in ordinary fluid motion, which are usually obtained by other methods.

Almost every paper in volume iv. is of a kind to arrest and hold the attention. The papers entitled hydrokinetic solutions are particularly interesting, both on account of their contents and from the fact that they are an excellent example of the best manner of the author, the manner of which the pupils of his higher mathematical class had the most frequent experience. It is probable, however, that the letter to Tait on the influence of wind on waves in water supposed frictionless, and on ripples, was written off as soon as some observations of the kind described in the letter had been made, probably in the yacht *Lalla Rookh*, and had been interpreted by the analysis written down immediately, and therefore in more or less impromptu manner. Scott Russell (though Lord Kelvin was not aware of the fact when he wrote) had noticed, nearly thirty years before, that surface tension must play the most important part in the propagation of the very short waves often seen on a smooth sheet of water; but this short paper proved that as the wave-length diminishes from a great to a very small value, the speed of propagation diminishes to a minimum for a certain wave-length, and then continually increases. This wave-length Lord Kelvin took as that of separation between ripples and ordinary waves.

Anyone can study the matter experimentally from a boat drifting steadily forward in smooth water at a speed of about half a mile per hour, while the wave system is generated by a fishing line stretched by a weight hanging in the water. How many physicists have done this and taken photographs of the wave patterns ("ripples in front and waves of the same velocity behind") with the excellent lenses and

cameras which are carried about on every holiday excursion?

The paper on an alleged error in Laplace's theory of the tides the present writer well remembers was dictated in some little excitement by Lord Kelvin just after he had read Ferrel's observations on Laplace's process in the United States Coast Survey Report for 1874, in which certain objections taken by Airy in his article on tides and waves in the *Encyclopedia Metropolitana* were quoted and approved. The point was a curious one, as to whether a certain coefficient K_4 was or was not indeterminate so far as the solution of the differential equation for certain tides was concerned. It is now generally admitted that the constant was not indeterminate, and is correctly determined by Laplace's "exquisitely subtle method."

Perhaps a note might have been added, in connection with the short paper in volume iv. on the precessional motion of a liquid, on the two liquid gyrostats which Lord Kelvin was so fond of showing to visitors. In each water is enclosed within a spheroidal shell, but in one the shell is oblate, in the other prolate, with about 5 per cent. deviation from sphericity in each case, and the shell rotates about the axis of figure. The motion of the liquid is stable in the first case, and the gyrostat takes precessional motion like an ordinary gyrostat, while in the other the motion is unstable. The first result showed that the precessional motion of the earth does not furnish a valid argument for the earth's rigidity. The remarkable result has been established that the motion of a liquid in a prolate case is stable if the degree of prolateness be so great that the axial length of the hollow filled by the water is more than three times the diameter. As Greenhill has pointed out, this fact is important in fixing the shape and size of the gas-bags for a dirigible balloon.

The most remarkable series of papers in the two volumes are, however, those on waves in water. Here the exponential solutions given by Fourier for the linear conduction of heat are modified and applied to the long chain of abstruse problems dealt with. The discussion of the front and rear of a free procession of waves, and the papers on the initiation and growth of a train of waves, sheds much light on difficult questions regarding groups of waves, and the manner in which they arise and are propagated from a given form. In the hands of Mr. Green and others it is possible that the analysis may have important applications in the wave theory of light, in connection with Lord Rayleigh's theory of the origin of the periodicity of light which has passed prisms and gratings and other optical instruments. A detailed account of these papers is a fit task for the specialist in hydrodynamics: that they should have been written by Lord Kelvin during the last ten years of his life is not the least wonderful thing about them. To those who came in contact with him, it appeared that he worked more slowly, but not less surely: the impetuosity of his genius was abated, not its natural force.

Of the papers in volume v. we have said nothing, and our space is exhausted. As a rule they are

shorter, and in some cases of a slighter character; for example, the excerpts from the technical journals on various questions of electrical practice; but the reader who wishes to follow Lord Kelvin in his more abstract speculations will find plenty of occupation in this volume also. There are his papers on molecular and crystalline theory, which are heroic attempts to find mechanical explanations of such things as the pyroelectricity and piezoelectricity of crystals, and the known chemical and physical properties of gases, liquids, and solid crystalline substances.

That one man should have attacked with so much success so many subjects of transcendent difficulty is a great marvel. But our wonder is mingled with regret that his mind should have been distracted so much as it was from the great themes with which it was pre-eminently fitted to deal. The turning from one thing to another probably, however, did not result in so much dissipation of its energy as might be supposed. He obtained mental refreshment by change of the objects of thought, and often when he returned to the consideration of difficulties obtained their solution with unexpected facility.

The work of editing has been excellently performed by Sir Joseph Larmor. He has appended notes here and there, but with a reverent frugality, and in their substance they are short and to the point.

A. G.

THE NUCLEI OF THE PROTISTA.

Die Konstitution der Protistenkerne, und ihre Bedeutung für die Zellenlehre. By Prof. Max Hartmann. Pp. v+54. (Jena: Gustav Fischer, 1911.) Price 1.60 marks.

THIS paper is an extended version of a lecture delivered by Dr. Hartmann at the International Zoological Congress in 1910. It is an attempt to obtain a simple interpretation of the many complex nuclear conditions recorded in the Protista.

By "constitution" Dr. Hartmann means "morphogenetic constitution," and it appears from his paper that "Protista" means—as is so usual—chiefly the Protozoa.

Dr. Hartmann's views are not easily given or criticised in a short space, for they contain much that is purely hypothetical and highly controvertible. He insists, in the first place, upon the universal occurrence of a centriole in all protists—a contention that is not likely to find universal acceptance. The cyclical changes (centrifugal and centripetal streaming) undergone by this body are also insisted upon.

Nuclei are classified as "monoenergid" and "polyenergid"—"energid" being used in a sense different from that of its inventor, Sachs. Monoenergid or univalent nuclei (e.g. in *limax* amœbæ) consist of two components—idio-generative (chromosomes) and locomotor-generative (centrioles, polar caps, &c.). The power of polar division is an important attribute of the locomotor component. Polyenergid nuclei (e.g. those of Radiolaria) are those which are really not simple, but multivalent, containing many individual-

ised univalent nuclei inside them. These are the "chromosomes" (e.g. in Aulacantha), which are really not chromosomes but nuclei.

Mitosis in the Protozoa is accordingly univalent or multivalent, according as the nucleus is monoenergid or polyenergid. From which it follows that the nuclei and their mitoses are not homologous throughout the Protista. There is no true amitosis in any protist—the process now so called being designated "promitosis."

We are glad to see that Dr. Hartmann now rejects the "box within box" trophokinetic binuclearity hypothesis which he formerly advocated so strongly, and now regards binuclearity—formerly of universal occurrence—as exceptional in the Protista. "Generative chromidia" he regards as monoenergid nuclei which have escaped from a polyenergid nucleus. The formation of new nuclei from chromidia is therefore "explained"—the nuclei are really there all the time, but at last become visible. There are also numerous other curious interpretations of protozoan nuclei resulting from Dr. Hartmann's hypothetical considerations. Thus, the mitotic figure of *Acanthocystis* is really not an ordinary mitotic figure, but a composite figure composed of two nuclei which simulate one. It may be noted, however, that the interpretation of monoenergid nuclei as consisting of two components is, in reality, merely a variant of Dr. Hartmann's former binuclearity hypothesis, become more complex by the introduction of polyenergid nuclei.

The relation of protozoan nuclei to metazoan nuclei and the constitution of the latter, is only briefly and hesitatingly considered. Dr. Hartmann thinks that all the chromosomes of a univalent protozoan nucleus are homologous with only one chromosome in a metazoan nucleus. Chromosomes in the former correspond with chromioles in the latter. Metazoan nuclei are, for the most part, polyenergid—the chromosomes being nuclei (or energids in Hartmann's sense). "Only the simple monoenergid atypical amœbæ and similar forms can be considered as elementary organisms in the sense of the cell theory." The union of the ovum and spermatozoon in Metazoa is not homologous with the conjugation of two protozoan gametes: for the latter are monoenergid, the former polyenergid.

Dr. Hartmann's ideas are discussed at some length in numerous special cases in the Protozoa, with the aid of figures copied from other works. There are several misprints in the text, and I may point out that "Herr Newin" referred to on page 18 is my friend and former pupil Mr. K. R. Lewin. His conclusions are at present indefinite, and do not warrant what Dr. Hartmann says of them.

Whether Dr. Hartmann's speculations will turn out to be well-founded or not, future work will determine. For our own part, we by no means agree with all his interpretations. But they will, no doubt, give rise to further discussion and analysis of a difficult problem, and thereby perhaps lead eventually to a better understanding of the nuclei of the Protista.

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